

The Eclipse Photographs

It would have given me much pleasure to have shown Mr. Winstanley the original negatives of the photographs of the late eclipse of the sun if he had called on me to see them, and by so doing he would have avoided falling into the mistakes which his letter contains.

At the time when the last photograph was taken the sky was perfectly clear, and unless Mr. Winstanley is in possession of exclusive information he has no right to assume that the American photograph was not taken under equally favourable conditions. Some of my photographs (which Mr. Winstanley cannot have seen) were taken through the edges of a cloud, the whole of which could be covered by the hand when held with the arm extended; and there was a perfectly cloudless sky near the sun excepting towards the east.

The imperfection in my No. 5 picture, which Mr. Winstanley's experienced eye detects, arose from the shaking of the telescope, caused by the high wind blowing at the time. Probably a single gust during the eight seconds while the plate was exposed caused the mischief, and this defect would never have been seen but for the extremely actinic power of the red prominences which leave their impression on the sensitive plate instantaneously. The moon's limb is perfectly sharp, excepting where the red prominences appear.

Let it be clearly understood that this "indifferent definition" refers to the moon's limb only; the details of the corona do not appear to have suffered; after the gusts of wind the telescope has returned to its proper position, and Mr. Winstanley must know from experience that the image of an object giving off feeble light would not be materially injured by a slight blow given to a firmly mounted camera.

Mr. Winstanley says that "the identity of the coronal rifts in the Cadiz and Syracuse photographs is not satisfactorily conclusive." Assertion is not proof. In NATURE of March 9 I gave evidence which appeared to me to be conclusive (I need not here refer to the opinions of others who are equally satisfied), and up to the present time no counter-evidence has been produced.

It is not for me to defend the American photograph. In due time we shall know all about how that was produced. But has Mr. Winstanley failed to notice that the light on the moon's disc does not extend all round and all over it as it would do if caused by our atmosphere? It is chiefly on the east and west sides. We may expect the explanation of this defect when we hear how it happens that the corona in this photograph is cut off instead of extending as in all the other photographs.

I fail altogether to see the connection between the solar corona and a lunar halo—the phenomena bear no resemblance to each other. The solar corona comes close up to the perfectly black disc of the moon. I never saw a lunar halo close up to the moon's limb. When seen through a mist or in a "sky burdened with innumerable clouds," there can be no doubt that the lunar surface is obscured by the moisture in our atmosphere.

A. BROTHERS

Ocean Currents

HAVING had occasion in the spring of 1868 to consider the subject of Ocean Currents as discussed by Captain Maury and Sir John Herschel, I was led to certain views respecting the origin of the oceanic circulation, which are briefly touched upon in a paper which appeared in the *Student* for July, 1868. At that time an experimental test of my theory (or rather of that portion of the theory I advocated, which was, as I judged, novel) occurred to me. The experiment might, I conceive, be very readily tried. It somewhat resembled that by which Dr. Carpenter illustrated lately at the Royal Institution his views respecting the influences of evaporation and polar cold; but as I wished specially to show how the westwardly equatorial current came about, the experiment was somewhat more complex. Let the circumference of a large and shallow cylindrical basin represent the equator and the central part the north polar regions. Within this cylinder let solid matter be so placed as to represent the northern halves of the continent, in such sort that the resulting configuration would correspond to that of a map of the northern hemisphere (say on the equidistant projection). Let sea-water be poured in to represent the northern portions of the terrestrial oceans. Now to represent the Arctic ice-fields, let lumps of ice be placed at the centre of the cylindrical vessel (they should be circled round by a wire-guard); and to represent the effects of equatorial heat, let

a stout iron ring round and above the rim of the cylindrical vessel be heated. In this state of things the process of circulation, which actually took place in Dr. Carpenter's experiment, would take place after such modified sort as the contour of the continent masses permitted. Now suppose that the cylindrical vessel is set in steady and somewhat slow rotation about its axis. It is clear that on the currents flowing from the pole and polewards, effects will be produced which precisely resemble those due to our earth's rotation. If I am right in regarding these effects as the true cause of the direction in which the equatorial currents, the Gulf Stream, and in fact all the currents in open ocean are observed to flow, abundant evidence to that effect will be obtained. If no such evidence be obtained, the westwardly direction of the equatorial currents must, I imagine, be ascribed to the trade winds, as Franklin and Sir J. Herschel have maintained.

In the summer of 1868 I suggested to Prof. Pepper that such a contrivance as the above, if it worked as I judged (and still judge) that it would, would form an interesting and instructive addition to the models exhibited at the Polytechnic Institution. Dr. Carpenter has already proved that the vertical circulation takes place in an experiment of this sort. If the eastwardly and westwardly circulation takes place as I expect, the experimental illustration of oceanic circulation would be singularly complete. The circulation in the southern hemisphere could be illustrated in like manner.

I may note here that the vast distance separating the polar from the equatorial regions must not be overlooked in theories respecting oceanic circulation. The influence of arctic cold may be paramount in very high latitudes; but equatorial evaporation must, it should seem, be the prime moving cause in tropical and sub-tropical regions.

RICHARD A. PROCTOR

Brighton, June 6

Day Auroras

I HAVE read attentively the numerous letters which have appeared in your columns on this subject; but so far as I can discern it seems not to have occurred to any of your correspondents that the auroral force, whatever it may be, affects every kind of cloud as well as the cirrus. On June 15, 1870, at 9 A.M., I witnessed here as complete a display of auroral motions in the cirrus cloud as ever I beheld in a midnight sky; and from that date I dismissed in my own mind all doubt as to the identity of auroral force, whether seen by day affecting the cirrus cloud or appearing as streams and rays of light at night. On Thursday and Friday last I witnessed a configuration of cirro-stratus cloud, evidently the result of magnetic polarisation, which I have no hesitation in characterising as auroral. There were on Thursday two poles, both in the line of the magnetic meridian; but on Friday night, at nine o'clock, only one pole in the direction of the true meridian. The phenomenon to which I refer is of very frequent occurrence, especially before a track of fine warm weather; and without at present offering a theory on the subject of auroras, I venture to class polarisations of clouds, whether cirrus or not, as arising from the same cause as luminous aurora. The transverse or dia-magnetic lines are generally as well marked in cloud auroras, and it is an interesting task to watch the transformation of cirrus cloud from the meridional to the equatorial direction. I have also noted that when these auroral lines converge towards the magnetic pole, a steady barometer and fine weather ensue; but that when the transverse or equatorial lines predominate and continue long visible, rain soon follows. These transverse lines of cloud are always lower and seem to be dia-magnetic.

D. LOW

Burntisland, May 22

PERHAPS you will allow me to add my mite to the discussion which has been going on in your pages on the question of the visibility of the aurora in daylight.

On the 3rd of September in last year, when at Nairn in the north of Scotland, I witnessed an aurora, such as I never heard or read of, or saw before; and strangely enough it was not noticed, as far as I am aware, in any of the newspapers. I had gone down to the beach at about 10.15 P.M., and immediately noticed what appeared at first to be a kind of haze over the whole sky, which slightly dimmed the light of the stars.

For a few minutes I thought no more about it, but, happening to turn my eyes towards the zenith, there was a sight I never shall forget. A number of sheets of whitish light were con-

stantly darting with a flickering motion from the surrounding haze of similar light, and meeting in the zenith; the length of their course was as much as 15° to 18° ; they appeared to proceed indiscriminately from all points in azimuth.

I immediately became aware that the whole sky, down to the very horizon, was illuminated by a white, colourless aurora; but I was so fascinated by the incessant play of the streamers overhead, that for some time I could notice nothing else. At last I turned away in order to observe accurately the full extent of the aurora in all directions. I found that it reached quite down to the horizon all round, except in one place, viz., in the S.S.E., and at that point there was a symmetrical arc (of a great circle, as far as I could judge) the summit of which was about 7° or 8° above the horizon. This arc was perfectly well defined; within it was blue sky, and above and around, over the whole heavens, nothing but the auroral light, except in the gaps between the darting streamers in the zenith. I carefully took the bearings of this remarkable arc, and found by means of a compass the next day, that it was bisected by the magnetic meridian. The phenomena underwent no diminution during the time (an hour and a half) I was watching it. The sky appeared to be quite free from cloud.

I have often witnessed fine displays of aurora; one in the winter of (I think) 1848, in this county, the colours and streamers of which were magnificent, far finer than those I saw on the 24th and 25th of last October at Edinburgh; but I never before observed the stars to be so much dimmed as they were at Nairn in September, notwithstanding the light on that occasion was colourless. On other occasions, I have always thought the stars quite unaffected by the auroral light, both to the naked eye and in the telescope, but on this they were obviously dimmed as by a haze.

My impression is, that no aurora that I ever saw could be visible in daylight, with the exception perhaps of this last, and the only portion of this that could possibly be seen in daylight was the well-defined arc low down in the S.S.E. I think it is just possible that in a clear and cloudless sky such an arc as this might be visible.

HENRY COOPER KEY

Stretton Rectory, Hereford, June 6

Red and Blue

I WAS much interested by the letter of Mr. T. Ward (*NATURE*, vol. iv. p. 68) describing the appearance of a blue colour when looking at white chalk marks on a black board while the sun was shining in the eyes, as I have frequently noticed a precisely complementary phenomenon.

While walking along the chalky roads of East Kent in bright sunshine, and reading under an umbrella, I have frequently noticed that the letters appear of a deep blood-red colour; the black colour of the type reappearing immediately on passing over the shadow of a tree on the ground, or on allowing the sun to shine directly on the book. This was so striking when first seen that I had to convince myself that the page was not printed in red ink. This is obviously the exact converse of the observation of Mr. Ward, who saw a blue colour from white marks on a black surface, while I saw a red colour from black marks on a white surface. A short time since I observed a precisely similar colour when looking down on the platform of a railway station with the setting sun shining on the eyes, the cracks between the boards also appearing red.

HERBERT M'LEOD

Influence of Barometric Pressure on Ocean Currents

A LOW barometric pressure and an increased height of the ocean was, I believe, first assumed to stand in the relation of cause and effect by Mr. Piddington. The abnormally high tidal waves that sometimes rushed up the Hooghly during Calcutta hurricanes were ascribed to the low pressures which accompanied them. There is no doubt that unequal pressure is a true cause of currents in the ocean. But I think it, as well as difference in specific gravity, may be regarded as infinitesimal in amount, compared to the influence of the winds.

The high tidal waves at the mouth of Hooghly are not experienced during the first stage of the hurricane, or so long as the wind blows from a northerly quarter. The waters in the Bay of Bengal are then propelled towards the south. It is only after the wind changes to the south and the barometer is rising that the waters are driven against its northern shores. It is when the

wind happens to change to the south at the time of the flow of the tide that great inundations occur in the Delta.

On the coast of the United States, however, there is apparently an intimate relation between low pressures and high tides. In general, so long as the barometer remains low, easterly winds are blowing on the coast and heaping up the waters in every bay. During the second stage of the storms the winds generally blow violently from the west or north-west, often at a right angle to the whole coast. These high westerly gales cause very low tides along the United States when the barometer is rapidly rising. But low tides are not experienced with high pressures if the air is calm.

High tides only occur on the coast of Europe after westerly winds have been blowing for some days in the Atlantic. It is a well-recognised fact among the fishermen on the east coast of Scotland that high tides are due to this cause. I think Hugh Miller was right in maintaining that the friction of the south-west winds on the wide surface of the Atlantic must be quite as powerful in maintaining the flow of the stream through the Florida Channel as the action of the Trade winds in forcing the tropical waters into the Gulf of Mexico.

Owing to the great rapidity with which barometric disturbances are propagated in our temperate latitudes, it is difficult to conceive how barometric pressure of itself can have an appreciable influence on the currents of the ocean. The rate of their propagation in winter is from thirty to sixty miles an hour. In the fourth number of the "Board of Trade Weather Report" an instance is given, in which the rate is affirmed to be upwards of seventy miles an hour. The velocity in this case, however, as I may try to show on another occasion, is estimated about ten miles an hour too high. But let us suppose that no winds accompanied these rapidly propagated depressions and elevations of the barometer. A difference of an inch of pressure existing between places on the ocean two or three hundred miles apart would create only a very slow moving current, even though the diminished area of pressure were stationary. But these low pressures pass so rapidly onwards that the *vis inertiae* of the waters of the ocean would hardly be overcome before they were again subjected to the opposite influence of an increase of pressure.

The effect of barometric pressure on the level of inland seas, like the Baltic and Mediterranean, must be still less than in the open ocean. Winds are often localised, but great depressions of the barometer extend over immense areas; in most cases far larger than the area of the Baltic. Any higher level from this cause would be brought about by the flow of the waters from either end, as the pressure might be assumed to be the same on both sides of that narrow sea. The mere effect of changes of barometric pressure, it will be admitted, would be quite inappreciable in any inland lake in Britain. But any one who is in the practice of fishing in the smallest of our lakes may always observe that there is an under current or "drag" created when the wind blows strongly towards the shore, in consequence of the accumulation there of its waters. The Niagara is sometimes suddenly raised two feet by strong west winds blowing over lake Erie. It is long since M. Volney, as regards the Mediterranean, stated that east winds caused a rise or flood of from two to three feet in the harbour of Marseilles, and that westerly winds produced opposite effects. It was by a careful deduction of effects produced on a small scale that this acute observer was enabled to give a consistent outline of the causes which produced the ocean currents in general. The currents of the ocean may be regarded as coinciding very closely with the average force and direction of the winds over its surface. Since, however, Humboldt assures us that the surface water of the Gulf Stream in the Florida Straits is sometimes reversed by the force of the winter "northers," it does seem vain to attempt to trace permanent surface currents in any part of the North Atlantic, vexed as its surface is by winds so inconstant in their force and direction.

Pilmuir, Leven, Fifeshire

R. RUSSELL

St. Mary's Loch, Selkirkshire

To the student of Nature it may seem easy to decide whether the water of any given lake is good for domestic uses. But as regards St. Mary's Loch, where the question has to be settled by dint of a squabble in the Auld Reekie municipality, with all its complementary dust, smut, and heat, the true aspects of Nature are liable to misrepresentation.

Although not resident in Edinburgh, nor subject to its prejudices and ratings, your correspondent has taken considerable